

C x R x 8 x
(2)

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Observations on the Results of an Advanced Diagnosis and Pathology applied to the Management of Internal Inflammations, compared with the Effects of a former Antiphlogistic Treatment, and especially of Bloodletting.* By J. HUGHES BENNETT, Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh.

(Read before the Edinburgh Medico-Chirurgical Society, Jan. 21, 1857.)

“Natura de ejusmodi methodo ac symptomatum concatenatione sibi prospexit, quibus materiam peccantem atque alienam, quae totius fabricae compagem aliter solveret, e suis finibus possit excludere. Quamlibet autem frequentius longe quam fieri cernimus, illum, ad quem remediis hisce ingratiss collimat, sanitatis scopum attingeret, nisi ab ignaris a recto quem tenet cursu detorqueretur.”—SYDENHAM.

DR ALISON, in a paper read to this Society last session, did me the honour of alluding, in very flattering terms, to a published introductory lecture of mine, on the “Present State of the Theory and Practice of Medicine.” On one point I had the misfortune to differ from him in opinion; and he has consequently re-stated the argument, and given additional illustrations of a principle he has sought to establish, which, if correct, as he truly observes, must be of great and general practical importance. But, for the sake of that truth to which he has appealed, I feel called upon to point out, what seems to me, another mode of explaining the change which has recently occurred in medical practice; I do this the more readily, as the profession, I trust, are now prepared to listen with calmness to those innovations on past experience, which the advanced state of pathology and diagnosis in modern times has rendered absolutely necessary. For it is a fact that can no longer be denied that our systematic works on the Theory and Practice of Medicine, are much at variance, both with our present experience and with our actual knowledge of the

nature of disease. Hence this discussion—and, assuredly, if past experience in medicine were asked to choose her champion, to none would she more confidently apply than to that distinguished physician and professor, who, for nearly half a century, has identified his name with the history of medicine in this school, and whose pupils most of us have had the high privilege of being. I sincerely regret that modern pathology has no better exponent of its views than myself; but supported by the conviction that it is to the advancement in medical science that we must look for its improvement as an art, I venture to enter upon a controversy, which, whatever be its issue, can scarcely fail, with so eminent and candid an opponent, to advance our knowledge of practical medicine.

Before entering into the argument, it may be well to state its nature. It is admitted by both parties that the practice of bleeding in acute inflammations, has within a recent period undergone a great change—that, whereas formerly it was the rule to bleed early, largely, and often repeatedly, that now such bleeding is rarely practised, and is not necessary. According to Dr Alison,¹ however, such remedies, but more especially blood-letting, were formerly highly successful by arresting the disease; whereas, now they are injurious, and fail to do so: and the inference he draws from these supposed facts is that inflammation itself is no longer the same; that its type, and more especially the febrile symptoms accompanying the inflammation, have altered from an inflammatory to a typhoid character, and that the practice, according to the correct therapeutical rules of our forefathers, has very appropriately changed with it. In short, it seems to be Dr Alison's opinion, that our advanced knowledge of diagnosis and pathology has had little influence in producing this great revolution in our treatment, but that the human constitution (in a manner which is not sought to be explained), is fundamentally altered, and that medical men were as right in bleeding twenty years ago, as they are correct in now abstaining from it. In opposition to these views, it will be my endeavour to show—1st, That little reliance can be placed on the experience of those who, like Cullen and Gregory, were unacquainted with the nature of, and the mode of detecting, internal inflammations. 2^d, That inflammation is the same now as it has ever been, and that the analogy sought to be established between it and the varying types of essential fevers is fallacious. 3^d, That the principles on which blood-letting and antiphlogistic remedies have hitherto been practised, are opposed to a sound pathology. 4th, That an inflammation once established cannot be cut short, and that the only object of judicious medical practice is to conduct it to a favourable termination. 5th, That all positive knowledge of the experience of the past, as well as the more exact observation of the present day, alike

¹ *Edinburgh Medical Journal*, March 1856.

establish the truth of the preceding propositions as guides for the future.

PROPOSITION 1.—*That little reliance can be placed on the experience of those who, like Cullen and Gregory, were unacquainted with the nature of, and the mode of detecting, internal inflammations.*

Inflammation for many years was generally recognised, especially in external parts, by the existence of pain, heat, redness, and swelling, and in internal parts by fever, accompanied by pain, and impeded function of the organ affected. In short, groups of symptoms, in accordance with the nosological systems of the day, constituted inflammation. But the school of morbid anatomy, by showing that inflammation was a diseased condition of a part, entirely overthrew the errors and confusion inherent in all such nosological systems. Clinical observation, based on a more correct diagnosis and pathology, has since demonstrated that artificial nosological groups of symptoms bear no relation whatever to the internal inflammations they were formerly supposed to indicate, and has led to a mass of information connected with internal disease, which, up to this time, has never been correctly systematised. Again, more recent histological research, by exhibiting to us that inflammation is in truth a disease of nutrition, governed by the same laws that determine the growth and functions of cells, as they exist in the embryo and in healthy tissues, and thus uniting physiology and pathology into one science, has removed our present knowledge still further from the traditional errors of the past. Why, then, should we, in our onward course, be governed by the opinions of Cullen and Gregory, of Gaubius and Sydenham, of Aretæus and Hippocrates? These distinguished men all pushed forward medicine in their day, as far as they were enabled by the then state of science and the means within their reach; but the principles which guided them ought no more to be considered laws to be followed now by practical physicians, than should the exploded astronomical doctrines of Copernicus and Tycho-Brache be acted on by practical navigators. It is not my intention, therefore, to enter into a lengthened refutation of the opinions of former writers, or even of many modern ones, in determining what pathologists now understand by the term inflammation. What I mean by it in the following remarks, is *an exudation of the normal liquor sanguinis*; and Dr Alison evidently means the same thing, when he acknowledges “that exudation of lymph is essential to almost all changes of structure produced by inflammation.” Whatever, then, may have been formerly understood by this word inflammation—in whatever way it may be now applied—whether to the congestion of the blood-vessels, the exudation of the liquor sanguinis, or to the change in the texture causing these phenomena—it is important to remember, that in speaking of it, both Dr Alison and myself mean a change in a part characterised by the

exudation of lymph through the walls of the minute vessels, resulting from changes more or less well marked in the nervous, vascular, sanguineous, and parenchymatous elements of that part, which it is unnecessary here to describe.

As regards diagnosis, it must be acknowledged by all parties that, up to a recent period, internal inflammations were sought to be recognised only by symptoms. But medical men, who have of late years studied these inflammations by physical signs as well as by symptoms, must have come to the conclusion that the latter alone are altogether insufficient to enable us to determine the existence of internal inflammations. This is a point which, if necessary, could be established by innumerable facts, showing, 1st, That where all the symptoms of inflammation have been present, yet *post-mortem* examination has demonstrated the absence of the lesion. And, 2dly, That inflammation has been the cause of numerous deaths, without one of the symptoms generally supposed to be its accompaniments having been present. But here, also, it is unnecessary for me to enter at any length into this question, because it is admitted by Dr Alison that we can now detect inflammation of the lungs "in cases where there is so little of pain, or cough, or dyspnoea, or inflammatory fever, that we should not in former times have given them the name of pneumonia." But when he goes on to say that "the cases of pneumonia thus overlooked, were attended with little or no immediate danger," I am constrained to dissent from this opinion, for it appears to me that many of these cases, especially such as are complicated and occur in old age (the so called latent pneumonias) are, at this moment, the most fatal, and that they always must have been so. On the other hand, the symptoms which formerly were supposed to indicate a pneumonia, viz., pain, cough, dyspnoea, rusty sputa, and fever, we now know are met with in a variety of lesions, independent of pneumonia, especially in certain cases of bronchitis in young subjects, or engorgements and apoplexy of the lung, associated with fever or heart disease in older persons. Hence, formerly, bleeding was not practised in many cases where pneumonia was present, whilst it was largely resorted to in others where that disease never existed at all.

Other writers besides Dr Alison have endeavoured to show, and not unsuccessfully, that what was formerly understood by pneumonia or peripneumonia, is altogether different from what we now mean by these terms. But they have not been so successful in deducing from the experience possessed by former physicians in treating symptoms, what ought to be the rule of practice for those in modern times who recognize the anatomical lesions of organs. If, indeed, it could be shown that the group of symptoms formerly called inflammatory, always indicated the same morbid lesion, former experience might still be useful to us. But we contend that this is what clinical observation proves to be impossible. Such are the contradictory statements and the confusion resulting from the unacquaintance of

the past race of practitioners with diagnosis and pathology, that no confidence whatever can be placed in their impressions, as to what cases are benefited by bleeding.

Hence, although I am far from repudiating experience in cases which in the present day are clearly recognizable as true inflammations, it is surely unreasonable to be guided by that experience in cases where it is acknowledged that the observations are imperfect and vague, and which, even among those who desire to take advantage of it, gives rise to endless differences of opinion as to what was meant or intended. Medicine is not a scientific art, which is dependent for its principles on the study of, and commentary on, the older writers. What *they* thought and what *they* said, are not, and ought not, in a question of this kind, to be our guide as to what was or is. On the contrary, it is the book of nature, which is open to all, that we ought to peruse and study, and why should we read it through the eyes of the sages of former times—when the light of science was comparatively feeble and imperfect—instead of bringing all the advanced knowledge of the present time, to elucidate her meaning. The lesson which a careful study of the history of medicine has forced upon me, is the necessity of re-investigating, with all our improved modern appliances, the correctness or incorrectness of existing dogmas, in order to establish an improved practice for the future.

PROPOSITION 2.—*That inflammation is the same now as it has ever been, and that the analogy sought to be established between it and the varying types of fevers is fallacious.*

The essential nature of inflammation has been already alluded to, viz., a series of changes in the nervous, sanguineous, vascular, and parenchymatous functions of a part terminating in exudation of the liquor sanguinis, or what some call effusion of lymph. How can it be shown that any of these necessary changes have of late years undergone any modification? If a healthy man receive a blow, or any other injury is inflicted on his person, are the resulting phenomena in these days in any way different from those which took place in the days of Cullen and Gregory? Are the effects which followed wounds received at the battle of the Alma, different from those which resulted from similar injuries at the battle of Waterloo? This has not yet been shown. Do we observe any essential difference in our civil hospitals in the effects of injuries, or in the process of healing, after wounds and operations? This also has not been shown. Again, if a healthy individual now-a-days be exposed to cold or wet, and be seized with an inflammation of the lungs or pleura, is not the lung hepatized in the one case, and do not layers of organizable lymph form in the other, in exactly the same way as formerly? If so, is not hepatization removed, and does not the lymph contract adhesions in the same manner now, as in the days of Cullen and

Gregory? If these changes have been materially modified in recent times, I again urge that such modifications have not been shown; and if they have not, in what can it be said that inflammation and its results have changed within the last twenty years?

To this question, notwithstanding repeated careful perusal of Dr Alison's paper, I am obliged to say I can find no answer. It is true he points out that the *symptoms* of the pneumonia of Cullen differ from those of the pneumonia of Grisolle. He also contends that it is only from the symptoms that we can judge of the effects of remedies. But before we can draw a comparison between variations in such symptoms as indications of the value of treatment, or found upon them a doctrine like the change of type in any given disease, it must be shown that the symptoms observed formerly, and those seen now, belong to the same lesion. No such comparison, however, can be drawn, because what Cullen meant by pneumonia were the symptoms themselves, whereas now such symptoms are known to be in no way necessarily indicative of pneumonia as previously explained.

Under these circumstances, nothing can be more unsatisfactory than to enter into an inquiry as to whether the inflammatory fever and hard pulse of Cullen's pneumonia—which may or may not have been pneumonia at all—does or does not differ from a true inflammation of the lungs, as we now recognise it. Dr Alison, by drawing a comparison between the two, seems at least to think they are allied, and he argues that the fever accompanying the one was inflammatory, whilst that accompanying the other is typhoid. Hence the reason why he thinks the first did, whilst the last does not bear bleeding. He has also long argued¹ that these differences are still observable in private or dispensary, and in hospital practice. But I have had abundant opportunity of satisfying myself that a true pneumonia is the same under every circumstance. During a seven years' attendance as physician to the Royal Dispensary of this city, I have seen pneumonia as typhoid as it can well be; and in the Infirmary, during the last nine years, I have seen it attack vigorous, healthy young men, and present all the characters of the inflammatory type. These last are exactly those cases which do best without blood-letting, whilst at the same time they are those also which bear bleeding well. The explanation of these (to some) apparently contradictory facts, will be given subsequently.

Another idea very extensively prevails on this subject, and is urged by Dr Alison, viz., that inasmuch as fevers undoubtedly present changes in type, inflammations may do so likewise. That essential fevers at different times are typhus, typhoid, or ephemerel, cannot be doubted, but this evidently depends on variations in the intensity or the nature of the exciting cause. On what these dif-

¹ *Outlines of Pathology and Practice of Medicine*, first edit., p. 221.

ferences depend is not yet determined. I have watched extensive epidemics of fever in France and in the Rhenish Provinces, where almost every case is typhoid and connected with intestinal lesion, and observed others in Edinburgh, where nearly every case is typhus, and free from organic lesion. I also noticed that, when owing to failure in the potato crop, as in 1846, the food of the people was materially changed for the worse, the fever in Edinburgh assumed far more of the typhoid type; and I have no doubt that changes in diet, in locality, in climate, in atmospheric influences, and a variety of causes, may induce modifications in fever. But surely no analogy ought to be drawn between the undoubted changes producing such varieties of fever, and those causing an inflammation, which, in all countries, and under every variety of external circumstance, are always the same. Even the results are said to be distinguishable only by a change in the force of the pulse.

But what should this asserted change in the force of the pulse indicate? It is said, that instead of being strong and hard in cases of pneumonia, as it used to be, it is now more soft and indicative of debility. Is it then argued that the whole people of this country, since the days of Cullen and Gregory, have become so debilitated—that their constitutions have been so altered for the worse, that attacked by the same lesion, and to the same extent, there is no longer the same reaction? and that the strong man of the present day, labouring under inflammation, presents the symptoms which twenty years ago distinguished the weak one? If so, where is the evidence of this? Are our soldiers and sailors, workmen or others, physically less capable of exertion than formerly? Is it true that the strength of mankind has so radically altered for the worse, during the last twenty years, as to account for the supposed fact that inflammation formerly required excessive loss of blood to check its progress, whereas now it stops of itself? For my own part, I have earnestly sought for, but cannot discover, a shadow of evidence for such a belief. Moreover, I have a most lively remembrance of all the facts and circumstances connected with the bleeding of many patients by myself, twenty-eight years ago, when I first commenced the study of medicine, as well as of such as took place in the Royal Infirmary of Edinburgh, when I was a student in this university in 1833; and my impression is, that not the slightest difference exists between the character of the pulse now and what it was then.

I have been informed by some Indian practitioners, that in the East blood-letting is now as little practised as it is among ourselves—so that, if Dr Alison's theory be correct, inflammation among the Asiatic nations and Hindoo tribes has undergone the same change of type, as is alleged to have taken place in Great Britain. But I have also heard that in Italy large blood-lettings are still practised as they were formerly; and I know from actual observation, that M. Bouillaud still pursues the *coup sur coup* treatment in acute

inflammations in his wards of La Charité Hospital Paris. On visiting him there last August, I saw several patients (all young persons) whom he had treated in this way, and who were then convalescent. On asking him whether he had observed any change in the character of the pulse, or a more typhoid character of the fever, in recent times, his reply was emphatically "certainly not." A similar reply was made to the question by every practitioner I interrogated in Paris, who attributed the general diminution of blood-letting in France to the gradual emancipation of medical men's minds from the doctrines of Broussais. Is it not more reasonable then to think, that the alteration of practice in India results from a change in precept and example, and that the continuance of the practice in Italy and in the wards of M. Bouillaud, is owing to the absence of such change, rather than to suppose that inflammation alters its type, just where the practice alters, but remains stationary in those countries, and even in those wards of an hospital, where it does not?

Hence, I am firmly of opinion, that inflammation in a part is the same now as it has ever been, and is only subject to the variations which occur in all diseases, such, for instance, as are dependent on differences of age, sex, vigour of constitution, complications, etc., etc. These are also alike at all times, and consequently the recent revolution in the treatment of inflammation by bleeding, cannot be accounted for by the theory of change of type.

PROPOSITION 3.—*That the principles on which blood-letting and anti-phlogistic remedies have hitherto been practised are opposed to a sound pathology.*

Large and early bleedings have been practised under the idea that by diminishing the amount of the circulating fluid—1st, The *materies morbi* in the blood would be diminished; 2dly, Less blood would flow to the inflamed parts; 3dly, That the increased quantity of blood in the part would be lessened; and, 4thly, That the character of the pulse was the index as to the amount of blood that ought to be drawn. Let us examine these four principles of practice—

1st, *Can the MATERIES MORBI in the blood be diminished by bleeding?* It was to its influence on the blood that the older writers especially attributed the good effects of venesection. This fluid, according to them, was thrown into a state of ebullition or fermentation, which worked off the morbid elements; and this termination was favoured by removing so much of it by blood-letting. On the other hand, if the morbid matters were not readily removed, they fell upon internal organs causing inflammation. This idea led to the abstraction of blood, under the notion that that fluid was diseased first, and local lesions supervened, as in the case of plague or small

pox. Thus Sydenham, apparently, had no idea of inflammation distinct from fever. He says, "I think pleurisy is a fever originating in a proper and peculiar inflammation of the blood—an inflammation by means of which nature deposits the peccant matters on the pleuræ. Sometimes she lays it on the lung itself, and then there comes a peripneumony. This differs from pleurisy only in degree. They exhibit the results of the same cause with greater intensity. In my treatment I have the following aim in view—to repress the inflammation of the blood, and to divert those inflamed particles, which have made an onset upon the lining membrane of the ribs (and have there lit up so much mischief), into their proper outlets. For this reason, my sheet anchor is venesection."¹ Such was the pathology and practice of Sydenham, the latter following consistently enough on the former; and the essential idea of diminishing the morbid matters in the blood has not only descended from Hippocrates to the days of Sydenham, but has come down from his to our own times.

Now, in one sense, it is true that there is no disease whatever, even the one which is most local, that is not also associated with a general change of the system. For, inasmuch as all the nutritive functions are connected with one another, an excess or diminution of local growth, by subtracting from or adding to the constituents of the blood, must produce an alteration in that fluid both as to quantity and quality. The idea of Treviranus, viz., that "each single part of the body, in respect of its nutrition, stands to the whole body in the relation of an excreted substance," has been ably shown by Mr Paget to account for various processes in health, under the name of "complemental nutrition."² The same notion has been still further extended by Dr Wm. Addison, who correctly points out that in the distinctive eruptive fevers, such as small-pox, the numerous small abscesses in the skin eliminate the morbid poison which formerly existed in the blood, and are in this way essential to the cure. This provident action he denominates "cell-therapeutics."³ In all such cases, experience has shown, that time and a natural sequence of changes is necessary for a restoration to health, and it is now recognised that the idea of cutting short such changes by bleeding, is alike erroneous in theory and injurious in practice.

Now, exactly the same principle ought to guide us in cases of inflammation, where, in addition to the local changes in the part, there must necessarily be more or less disturbance of secretion and excretion. The blood in consequence must, and does, as is now well known, undergo definite alterations, which, it is true, organic chemistry has not yet fully explained to us, but by which we have at least learnt that the particular secretion suppressed is always accu-

¹ *Sydenham Society's Translation.* Vol. i., p. 247.

² *Lectures on Surgical Pathology,* Lecture ii.

³ *Addison on Cell-Therapeutics,* 1856.

mulated in the blood, which also contains an excess of fibrin. The careful investigations of chemists, and especially those of Andral and Gavarret, Simon, Becquerel and Rodier, and others, have further shown us, that whilst venesections greatly deteriorate the blood, rendering it poorer in corpuscles and richer in water, that they have no effect on the fibrin whatever. It follows that an elimination of the morbid products can be accomplished in inflammation only by the conjoined action of cell growth in the part, and a peculiar vital chemistry going on in the blood (as will be subsequently explained), neither of which can in any way be facilitated, but, on the contrary, are both, in the vast majority of cases, impeded by blood-letting.

2d. *Is it good practice to diminish the flow of blood to the part?*—The increased throbbing and circulation of blood in the neighbourhood of an inflamed part may be shown not to be the cause of inflammation, but the result of it. The idea of so-called determination of blood being the cause of disease is thus set forth by Dr C. T. B. Williams:¹—“In the frog’s web, gently irritated by an aromatic water, we see the arteries become enlarged, supplying a larger and more impulsive flow of blood to the capillaries and veins, which all become enlarged also; and the whole vascular plexus, including vessels which before scarcely admitted red particles, then become the channels of a much increased current. This is determination of blood.”—P. 203. Again, “We may affirm, from direct observation, as well as from reasoning, that determination of blood is caused by enlargement of the arteries; and this enlargement is the effect of the pressure of the arterial distension from *behind* acting on a tube, which has lost some of its contractile power.”—P. 203. Again, “One patient was subject to attacks of determination of blood, which caused him so much suffering and loss of moral control, that he cut his throat to destroy his life. When recovering from the wound, attacks sometimes came on; *first* with beating of the carotids, then flushing of the face and head, suffusion of the eyes, and sensations of distraction in the head.” “Fits of epilepsy and convulsive hysteria are immediately *preceded* by throbbing of the carotids, which shows that determination of blood is the proximate cause of the paroxysm.”—P. 201. Now, if I am correct in supposing that the meaning of these passages is, that the larger arteries assume the initiative, take upon themselves the action of a forcing pump, and send or determine more blood to the part, then it appears to me there must be error both in observation and reasoning. So far from the enlargement of arteries and increased current preceding the changes going on in the capillaries—so far from being connected with the causes of inflammation—I venture to affirm that they are the results.

In all cases, the primary stimulus producing inflammation is

¹ *Principles of Medicine.* (Third Edition.)

applied to the vessels of the part, either directly (as from injuries or irritants) or indirectly, that is by reflex action (as in the case of internal inflammations following exposure to cold, etc.); and in consequence, that is to say, as a *result* of the local change in the part thereby occasioned, there follows the throbbing of the neighbouring arteries. Let us attend to what takes place in the finger from a thorn entering the skin, and which is not extracted; the irritating body first acts upon the nerves and blood-vessels of the part, then comes on the stoppage of blood and exudation, and lastly follows the throbbing of the artery in the finger. Surely this throbbing, which is the evidence of so-called determination, is a result of the inflammation, and not a cause of it. The blood in this case, instead of being sent by a *vis a tergo*, is in fact drawn by a *vis a fronte*, and, as we shall endeavour subsequently to show, for the most important purposes. Whether would it be reasonable to treat such an inflammation by opening the artery, or by favouring the transformation of the exudation thrown out into pus, whereby the irritating cause and its results are both got rid of? All experience shows that the latter is the proper treatment, and that wounding the artery under such circumstances would probably be highly injurious by producing gangrene.

But why should nature, in cases of inflammation, draw an increased amount of blood towards the part? She does so, it seems to me, in obedience to one of her wisest laws, but one which has been too much ignored by medical practitioners. It must be obvious, however, that an inflammation having occurred, the great work now to be accomplished is an increased growth by cell formation, whereby that exudation is to be broken up, the pressure it exerts on the nerves and blood-vessels removed, and the whole rendered capable of being eliminated from the economy, either directly by discharge externally, or indirectly, 1st, by passage into the blood, and, 2d, by excretion through the excretories. To perform this work of increased growth, an augmented flow and amount of nourishing fluid is necessary, the same as is observable at the period of heat in animals, to ripen the Graafian vesicles; in the stag's scalp during the growth of the antlers; in the mamma when milk is first secreted; in the gums during the process of dentition; in the ascent of the sap during spring in plants, etc., etc. In all these cases, especially the last, the fluid is not sent or determined, but drawn to the part, in consequence of the increased growth of cells imperatively requiring a greater amount of blastema. So in inflammation, an exudation having been poured out, which has to be transformed by a process of cell growth—in order that it may be removed or rendered subservient to the wants of the economy—it is absolutely imperative that the part in which these nutritive changes go on should receive more blood, to enable it to accomplish them. Hence the increased current. But hitherto medical practitioners have supposed that this phenomenon is injurious, and ought to be checked by blood-letting

and antiphlogistics. The rapid flow of blood, which is so necessary, they have sought to diminish; and the increased amount in the neighbourhood of the part, which is so essential for the restoration to health, it has been their object to destroy. In doing so, we argue, they act in opposition to sound theory, and, as we shall afterwards attempt to show, to good practice also.

3d. *Can general blood-letting diminish the amount of blood in the inflamed part?* It is vain to deny, that the notion of lessening the amount of blood in the inflamed part has constituted one of the chief reasons for blood-letting, and given rise to long discussions as to whether this or that vein shall be opened, and whether leeches shall be applied to the occiput or to the feet. Now, it requires to be shown that draining the body of blood can in the slightest degree influence the congestion in the inflamed part. *There* the vessels are enlarged, the current of blood is arrested, the blood-corpuscles are closely aggregated together and distend the vascular tube, and are in no way affected by the arterial current, even when increased in its neighbourhood. That opening a vein can alter this state of matters is scarcely to be conceived; and if it could, how would this assist in removing the exudation, which has coagulated outside the vessels?

A consideration of the connection and distribution of the large vessels in the body will still further show the little probability there is of either general or local blood-letting, as usually practised, being capable of influencing the amount of blood in the part actually inflamed. How is it possible, for instance, that venesection in the arm can *directly* diminish the amount of blood sent from the heart by the great pulmonary artery to the lungs, by the carotids and vertebrals to the brain, or by the coronaries to the heart itself. In inflammation of those organs, blood-letting, to be of any use, must be large, so as to affect the general system *indirectly*, by weakening the heart's action and producing syncope, and this at a time when, from no nourishment being taken in consequence of fever, great prostration of the vital powers is to be expected. But whilst these effects may certainly be induced by large bleedings, the inflammation in the part is altogether unaffected. The exudation, under such circumstances, which requires more blood in order that it may undergo the necessary transformations previous to removal, is then often arrested in its development, and, so far from being rapidly removed, remains stationary, or dies in proportion as the economy is exhausted. If, on the other hand, small or moderate bleedings are practised, how can they operate even on the principles of those who advocate them? These do not affect the heart, or lower the force of the circulation, even in the neighbourhood of the inflamed part; how, then, can they operate on the stagnant blood in the inflamed part itself?

As to local bleeding, its supposed effects are inexplicable on the

supposition of drawing blood from the inflamed internal parts. A man has pneumonia or nephritis, resulting from changes in the vessels which are supplied direct from the aorta, and leeches are applied to the integuments supplied by vessels derived from the mammary or epigastric arteries. Any direct anastomosis between the vessels on the surface and those in the parts inflamed is not to be thought of, as has been shown anatomically by Dr J. Struthers.¹ How, then, does the loss of this small amount of blood operate in these important cases? It cannot be by any conceivable theory of diminishing either the current to, or the amount of blood in, the part. As in the majority of cases, therefore, the loss is not large enough to affect the general circulation, and as anatomy prevents our belief in the idea that it can influence the inflammation directly, it may well be asked how can local blood-letting be of any benefit at all? Is it not more probable that leeches and cupping do no good by drawing blood, but that the relief of pain they undoubtedly occasion is owing to the reflex influence of counter irritation, and in the vast majority of cases to the soothing and sedative influence of the warm fomentations which generally follows their employment? Dry cupping is often as effectual as local blood-letting.

From these considerations, it follows that neither general nor local blood-letting can possibly be supposed to diminish the amount of blood in internal parts affected with inflammation.

4th. Should the character of the pulse indicate the propriety of bleeding? That an accelerated and strong pulse in inflammation demands interference on the part of the medical practitioner, seems to be a principle which has been very generally acted on. In other words, because nature accelerates and strengthens the pulse, it has been thought that art ought to interfere and diminish its force and frequency. But here, again, as it appears to me, the result has been mistaken for the cause; and so far from getting rid of inflammation by weakening the pulse, we not only fail to do so, but prolong the time, as previously explained, for the transformation of the exudation. This, indeed, is acknowledged by Louis, Chomel, and Grisolle, who distinctly show that the progress of a pneumonia is never shortened by bleeding. Dr Alison also admits that he has seen dulness with crepitation continue to spread *after* bleeding. But the inconsistency of the therapeutical rules on this head will become more manifest when we remember that it is necessary, in the opinion of many medical practitioners, not only to weaken the pulse when it is strong, but to strengthen it when it has been made weak. Now, although it is obviously good practice to support the strength when the calls upon the nutritive functions have exhausted the economy, it is injurious to diminish by bleeding the nutritive processes themselves, when they are busily engaged in operating on

¹ *Anatomical and Physiological Observations.* Edinburgh, Svo, 1854.

the exudation and eliminating the morbid products. In short, we argue that the phenomena of fever and excitability following inflammation have been wrongly interpreted, and that danger is to be apprehended from them not directly, but from the subsequent exhaustion which all great exertions of the animal economy produce. In themselves, these are sanative, and indicate the struggle which the economy is engaged in when attempting to get rid of the diseased processes; and we only diminish the chances of that struggle terminating favourably by lessening the vital powers at such a critical juncture. This proposition seems to be universally admitted in the case of essential and eruptive fevers; and exactly the same rule ought to apply in cases of inflammation.

It has been argued, however, that the relief which blood-letting produces justifies the practice. But this is a therapeutic question of the greatest importance, and one which, I venture to think, has not been sufficiently considered by medical men. No doubt pain is a great evil; and mankind instinctively seek for its relief, and sometimes at any cost. But, on the other hand, the possession of life is sometimes only to be maintained at the price of suffering more or less privation and pain. It is in this point of view that disease may frequently be considered as a benefit and a great good, mercifully sent by a wise Providence to reconcile man under a variety of circumstances to death itself, as to a great relief. But such is not the therapeutic or curative method of considering the question; the great object of the physician being *first* to cure, and, should his attempts in that direction fail, *then* to relieve. If both objects can be accomplished, so much the better; but if the means of relief are opposed to those of cure, then to obtain the latter the former must be unhesitatingly sacrificed. I have pointed out in another place how much this principle has been overlooked in the treatment of pulmonary tuberculosis;¹ and in no case does it appear to have been more disregarded than in inflammation. For assuming it as granted that in some cases the pain is for a time relieved by bleeding, and that in pneumonia the respiration temporarily becomes more free, at what a cost are these advantages obtained, should the patient be so weakened as to be unable to rally. Even if he does rally, a large bleeding almost always prolongs the disease. Of course I am now speaking of a true pneumonia, and not of that combination of symptoms which was *called* pneumonia by Cullen and Gregory. I do not consider it necessary to cite cases in proof of the fact that in many instances bleeding has done great mischief, because this will be readily admitted by all candid medical men.

But whilst large and repeated bleedings, practised with a view of arresting the disease, appear to me opposed to a correct pathology, small and moderate bleedings, directed to palliate certain symptoms,

¹ The Pathology and Treatment of Pulmonary Tuberculosis, by the Author. Edinburgh, 1853. p. 84, *et seq.*

and especially excessive pain and dyspnœa, may reasonably be had recourse to, and unless there be great weakness, without any fear of doing injury. I have often been struck, especially in cases where large thoracic aneurisms cause these symptoms, with the small loss of blood which will occasion marked relief. The same result may be hoped for in other cases where the congestion is passive, even when that is associated with active repletion of blood, followed by exudation. But I need scarcely remark that this mere palliative object of blood-letting is not the ground on which the practice has hitherto been based, and that in this point of view it requires to be very differently explained. To this subject I shall again allude. In the meantime, it follows from the arguments which have been adduced under the present head, that the past principles which have indicated the practice of bleeding in inflammations are erroneous. It now remains for me to advance and endeavour to establish to the satisfaction of my hearers what appear to me to be the true principles of treatment.

PROPOSITION 4.—*That an inflammation once established cannot be cut short, and that the only end of judicious medical practice is to conduct it to a favourable termination.*

There was a time when it was supposed that typhus fever, small pox, and many other diseases, which are now always allowed to run their natural course, were curable by medical interference. But with regard to them, there have been established the principles, 1st, of prevention, and 2d, when this fails, of simply conducting them to a favourable termination. It appears to me that the same rule ought to hold with regard to internal inflammations, and that this will be admitted when it is made apparent, not only that every inflammation once formed runs through a definite course, but what that course is. This I now proceed shortly to consider.

If, then, we watch the natural progress of inflammation in any of the textures of the body, we observe that it terminates in two ways, viz., 1st, by vital changes of growth of different kinds in the exudation, constituting what has hitherto been called suppuration, adhesion, granulation, cicatrization, the healing processes, etc., etc.; and, 2dly, by death of the exudation, which, if rapid, putrifies, producing gangrene, or, if slow, disintegrates, causing ulceration. The first series of changes are not destructive, but formative and reparative. Suppuration especially should be looked upon as a kind of growth, which enables the exuded and coagulated blood-plasma to be rapidly broken up, and eliminated from the economy. If so, instead of being checked, it should be encouraged as much as possible; a very different doctrine from what has hitherto prevailed. Again, every thing that lowers the vital strength and weakens the economy, must impede the nutritive processes of growth, and tend more or less to a slow or rapid death of the exudation. Blood-letting, espe-

cially, has this tendency, and must, therefore, be wholly opposed to the rapid disappearance of inflammation ; for example :—

If a bone be fractured, inflammation occurs around the injured part, and exudation is poured out, which undergoes vital changes, whereby ultimately it is transformed into bone. If soft parts are destroyed or removed, the exudation poured out from the injured vessels undergoes other vital changes, whereby it is transformed into fibrous tissue, constituting first granulations, and then a cicatrix. After subcutaneous section of tendon, with separation of its extremities, the transformation is more perfect, producing, as in the case of bone, a growth exactly similar to the one which was injured. If a violent blow or injury has been received, a greater or less amount of exudation is infiltrated among the contused and torn tissues, which is transformed by cell growth into pus, which, if it can be evacuated externally, is soon got rid of, but if not, is on the disintegration of the cells absorbed and excreted from the economy. If, under other circumstances, the pus is absorbed as rapidly as it is formed, the inflammatory swelling is said to be resolved or discussed ; if not, it collects in the form of a fluid to constitute an abscess. Surely it cannot be maintained that, in any of these cases, we can favour these reparative processes by blood-letting and lowering the strength of the economy. On the contrary, they have always been found to be best perfected in individuals of vigorous constitutions, whilst in scrofulous or broken-down and weak persons, they proceed slowly or not at all.

But in internal inflammations, say of the lungs or pericardium, are the processes different ? Certainly not. In the one case the exudation is converted into pus cells and absorbed, and in the other into fibrous texture, causing adhesions. But because these processes have been hid from view, physicians have supposed that, instead of treating the inflamed parts as the surgeon does, he ought to attack the general symptoms which result from the lesion. In cases of fracture and contusion, there are also febrile symptoms, increased pulse, and so on. But does the surgeon imagine that callus will form better, or his abscess be resolved, or reach maturity sooner, by general blood-letting and antiphlogistics ? Experience teaches him otherwise, and in the same manner it is certain that such treatment does not favour the natural termination of internal inflammations.

It may be well, however, in further proof of this, to point out a little more particularly what are the changes which a pneumonia and a pericarditis do go through, as illustrative of the proposition we seek to establish.

In pneumonia the exudation is infiltrated into the air vesicles and minute bronchi, and between the fibres, blood-vessels, and nerves of the parenchyma, imprisoning the whole in a soft mass, which coagulates and renders the spongy texture of the lung more dense and heavy, or what is called hepatized. This accomplished, no air can enter, the circulation in the part is arrested and the nerves com-

pressed, and the object of nature is now to reconvert the solid exudation once again into a fluid, whereby it can be partly evacuated from the bronchi, but principally reabsorbed into the blood, and excreted from the economy. This is accomplished by cell-growth. In the amorphous coagulated exudation, granules are formed, around groups of these cell-walls are produced, and gradually the solid amorphous mass is converted into a fluid crowded with cells. This is pus. The cells, after passing through their natural life, die and break down, whereby the exudation is again reduced to a condition susceptible of absorption through the vascular walls, and once again mingles with the blood, but in an altered chemical condition. In the blood, the changed exudation (now called fibrin) undergoes further chemical metamorphoses, whereby, according to Liebig, it is converted by means of oxygen into urate of ammonia, choleic acid, sulphur, phosphorus, and phosphate of lime. The urate of ammonia, by the further action of oxygen, is converted into urea and carbonic acid; the choleic acid into carbonic acid and carbonate of ammonia; the sulphur and phosphorus into sulphuric and phosphoric acids, which, combining with an alkali or earth, form sulphates and phosphates. If it should happen that the quantity of oxygen taken is not sufficient completely to accomplish this cycle of changes, then, instead of urea, either urate of ammonia appears in the urine, or if the ammonia have entered into any other combinations, pure crystals of uric acid or fibrin. In consequence of these or similar changes, the exudation is finally removed from the economy.

In a pleurisy or a pericarditis, the transformations occurring in the exudation are different. Let us follow them in the case of pericarditis, as we have carefully described them in pleuritis in another place.¹ When a severe inflammation of the pericardium occurs, the liquor sanguinis is exuded in considerable quantity, separating the serous layers to a greater or less extent. After a time the fibrin coagulates and forms a layer which attaches itself to the membrane, whilst the serum of the blood accumulates in the centre. The coagulated fibrin at first assumes the form of molecular fibres, plastic or pyoid cells are formed in it; others throw out prolongations, so as by their union to form a plexus, which, communicating with the vessels below the serous membrane, renders the exudation vascular. Gradually the surface assumes the appearance of a villous membrane, as well as the absorbent functions of one. The enlarged villi frequently contain vacuoles or spaces, reminding me strongly of the placental tufts, than which nothing can be imagined more perfectly adapted for the purposes of absorption. In consequence, the serum now disappears, the two false membranes are brought into contact, and thus absorption as soon as it is no longer required is put an end to, and adhesion occurs. The matters absorbed into the blood pass through the same series of changes as those in pneumonia do, and

¹ *Lectures on Clinical Medicine.* Edin., 1856. P. 256, *et seq.*

are eliminated from the economy in a similar manner. Such is the natural progress of pericarditis.

The two kinds of processes now described exhibit the same wise design in pathological as we everywhere find in physiological actions. In the vascular tissue of the lung, new blood-vessels are unnecessary. But in the non-vascular serous membrane, they must be formed to bring about removal of the morbid products. In the one case the entire exudation is transformed into cells, to produce rapid disintegration and absorption, which latter is easily accomplished by the already formed numerous vessels of the lung. In the other case, the exuded liquor sanguinis is separated into solid and fluid parts, and as there are no vessels in the serous membrane, they are formed in one portion of the exudation to cause absorption of the other.

During the progress of these essentially vital acts and modes of growth and formation, how can it be supposed that lowering the strength by blood-letting can influence them in any way except for the worse; that is to say, weakening that power on which the transformations depend? Dr Alison admits that "it is not merely the mechanical change of position of many particles of the blood, but a strictly vital action, such as we trace up only to principles of physiology and pathology, to which we have to apply a remedy." The nature of this vital action he has not sought to explain. But if, as we have pointed out, it is essentially a formative one, and in kind identical with growth in young tissues, it ought not to be diminished or destroyed by depleting remedies.

But, says Dr Alison, if we abandon blood-letting, as recommended by so many practical authors in obedience to this doctrine, "we shall be trusting to a pathological view of a vital process, still very imperfectly known, in opposition to a therapeutical principle, founded, no doubt, on empirical observation only; requiring, no doubt, as all powerful remedies do, an exercise of judgment of the practitioners in applying it; because it may easily do harm by enfeebling, and at the same time rendering more irritable, all the vital actions involved in the disease, more than good, by restraining the amount of the exudation; but, nevertheless, much more to be depended on, *as guiding practice in these circumstances*, than any principle in pathology." If, however, instead of being imperfectly known, as is supposed, it should turn out that the pathological view I am contending for is true, and is extensively known among the younger members of the profession, then the admission here made by Dr Alison of how easily blood-letting may do harm and enfeeble, may be expected to produce an effect prejudicial to its employment. Besides, the moment a pathological law can be successfully established, empirical rules are overthrown. Dr Alison, who has done so much in attempting to establish the supremacy of vital laws, is too good a logician not to know this. Hence his objection is directed against the uncertainty and want of general information of the theoretical view as a guide to practice, when compared with the advan-

tages which he considers the empirical rule has produced, as tested by past experience. This, then, leads me to abandon pathological research and deduction, and inquire how far actual facts indicate which is the best practice, blood-letting in obedience to empirical rules, or abstaining from it, in accordance with the pathological principles now brought forward.

PROPOSITION 5.—*That all positive knowledge of the experience of the past, as well as the more exact observations of the present day, alike establish the truth of the preceding principles as guides for the future.*

In endeavouring to determine from experience what is the value of bleeding in acute inflammations, it must be remembered that, whilst past experience has declared it to be the *sine qua non*, the *summum remedium*, the only certain means of cutting short the disease, and so on—present experience declares by almost universal consent that now-a-days individuals labouring under them recover rapidly without bleeding at all. This admission constitutes the basis of the theory advanced by Dr Alison, viz., that acute inflammations within the last twenty years have changed their type. So that the question now is not whether no bleeding is good practice, but how the admission of this fact is to be reconciled with the experience of twenty years ago. But inasmuch as for the reasons previously given, we cannot suppose for a moment that inflammation has ever undergone any change whatever among mankind, it necessarily follows, if modern practice in this matter be correct, that former bleedings must have been inert or injurious.

Before it is possible, however, to determine with exactitude the value of any practice, it is essential to ascertain the natural duration of the disease we propose to treat. Fortunately, we have now some data which will enable us to arrive at this information with regard to one of the most frequent and important kinds of inflammation known to us, viz., pneumonia. Very severe cases of this disease were observed by Dr George Balfour of Cramond, in the Homœopathic Hospital of Vienna, under a treatment that no reasonable medical man can suppose to be anything else than inert. Yet most of these cases got well, and may be considered as excellent studies of the disease left entirely to nature.¹ We have also the accounts of the expectant systems of treating this disease in Vienna, under Skoda² and Dietl.³

From all the accounts which have been published concerning the natural progress of a pneumonia, it would appear that very slight cases (that is, where the inflammation has been limited) may be

¹ *Brit. and For. Medical Review*, vols. xxii. and xxiii.

² Dr G. Balfour in *Edin. Med. and Surg. Journal*. 1849.

³ *Der Aderlass in der Lungenentzündung*. Wien. 1849.

convalescent on the seventh day; that the majority of cases of medium intensity recover between the seventh and fourteenth days, and very severe ones between the fourteenth and twenty-first days. It is sometimes difficult to judge from the accounts of different authors what, according to them, constitutes the number of days an individual is affected. Some suppose that disappearance of fever, or cessation of pain, marks recovery. Others have declared convalescence to be established as soon as the patient can take a little beef tea. I have ventured, however, to name periods between the seventh, fourteenth, and twenty-first days, from a careful examination of the cases themselves, and from applying to them the rules which have governed the records of my own practice. These are to consider the commencement of the disease as indicated by the rigor, and the termination as indicated by marked diminution in the physical signs conjoined with disappearance of the leading symptoms,—I say marked diminution, because a certain amount of increased vocal resonance, and even crepitation, not unfrequently continues for some time after the individual has left his bed, and has, to all intents and purposes, perfectly recovered. Such, it appears to me, is the average duration of pneumonia, and I believe that if an individual can be shown to have recovered from an attack of the disease which has involved, say two-thirds of one lung in fourteen days, it is a good recovery, and yet only consistent with the natural progress of the disease in sound constitutions.

Here it is important to consider that the violence of the symptoms bears no necessary relation to the extent or intensity of the disease. Some persons present great fever and constitutional disturbance when one lung is only slightly involved, and recover rapidly; whereas others may have an entire lung inflamed, or portions of both lungs, and exhibit comparatively trifling fever and few marked symptoms, until impeded respiration occurs, ushering in death. It is a knowledge of this important fact which serves to clear up much of the discrepancy existing between past and present practice, especially when conjoined with another, viz., that however bleeding may relieve symptoms, it has no influence in shortening the duration, or diminishing the extent of the disease. Of this fact, the observations of Louis, Grisolle, and Alison, can leave us in no doubt; and I have frequently satisfied myself of their correctness. It follows that, as the past race of practitioners considered pneumonia only demonstrable by symptoms, which furnished the sole evidence of the advantage of bleeding, that as soon as these symptoms were diminished by venesection, they thought benefit was accomplished. Whereas now it has become apparent that such is no certain evidence of recovery from the disease, which may linger notwithstanding for weeks, give rise to a tedious convalescence, and even induce death by exhaustion after active functional symptoms have for the most part disappeared.

The real tests of successful practice, therefore, are not to be sought

for in the relief of symptoms, but in the removal of the disease; and that treatment will be best which, *cæteris paribus*, causes fewest deaths, and recovery in the shortest time. Let us, then, look at the results, 1st, of the antiphlogistic treatment as formerly practised by bleedings and tartar emetic; 2d, of the expectant system, or what ought to be called a dietetic system; 3d, of the treatment directed to further the natural progress of the disease as I have explained it.

Treatment by Bleeding.—The published statistics of the Royal Infirmary of Edinburgh present the following results:—

TABLE showing the number of patients affected with pneumonia treated in the Royal Infirmary of Edinburgh, and the results, from July 1, 1839, to October 1, 1849:—

Total No. of Patients entering the Infirmary.	Years.	No admitted.	Cured	Relieved.	Died.	Statistician.
7969 ¹	1st July 1839 to 1st Oct. 1841	139	85	5	49	Dr John Reid. Dr T. Peacock.
3537	1st Oct. 1841 to 1st July 1842	42	23	3	16	
2760	" 1842 " 1843	41	26	0	15	
7204 ¹	" 1843 " 1844	31	16	4	11	
3252	" 1844 " 1845	50	33	4	13	Dr Hughes Bennett.
3638	" 1845 " 1846	61	40	6	15	
7435 ¹	" 1846 " 1847	93	47	5	41	
7446 ¹	" 1847 " 1848	103	52	6	45	
3724 ²	" 1848 " 1849	88	66	5	17	Mr McDougall.
46,965		648	388	38	222	

It appears from this account, that upwards of one-third of all the patients affected with pneumonia, who entered the Infirmary during a period of ten years, died. No doubt, it cannot be pretended that perfect accuracy as to diagnosis was attained in all these 648 cases. It is certain, also, that numerous complications, and the debilitated constitutions so frequently met with in the practice of a large hospital, served to swell the mortality. It is remarkable, however, that this proportion of deaths to recoveries is nearly the same as has occurred in the Infirmary since the commencement of the present century, as well as what resulted in the cases so carefully observed by M. Louis, in the hospital of La Charité, at Paris. Thus, my late resident clerk, Dr Thorburn, was kind enough, at my request, to go over 208 case-books of the Infirmary, belonging to the years 1812 to 1837, belonging to twelve physicians, all of whom practised an antiphlogistic treatment. He found that of 103 cases of pneumonia, 55 were cured, 41 died, and 7 were relieved. Dr Thorburn

¹ At these periods there were great epidemics of fever.

² At this period considerable changes took place among the medical staff of the Infirmary.

then carefully read over these 103 cases, and rejected all those that were incomplete or which presented no evidence of having been pneumonia. The remainder were tabulated, and it may safely be said that they were all cases of pneumonia, or of acute inflammations of the chest, closely allied to that disease, and the result was:—Number of cases, 50; died, 19; cured or relieved, 31.

The total number of cases recorded by M. Louis, was 107.¹ Of these 32 died, or 1 in $3\frac{1}{3}$. In 78 of those cases which occurred at La Charité, bleeding was performed from the first to the ninth day, and the deaths were 28, or 1 in $3\frac{1}{7}$. The duration of the disease in the cases which recovered, was $15\frac{1}{2}$ days. Of the remaining 29 cases, which occurred at La Pitié, the bleeding was performed earlier, that is, during the first four days, and of these only 4 died, that is, 1 in $7\frac{1}{4}$. The duration of the disease, however, in the cases that recovered, was $18\frac{1}{4}$ days. This diminished mortality, but greater length of recovery, M. Louis attributes to the bleedings not having been so large, and the greater amount of tartar emetic employed. Hence, the proposition he sought to establish, that although bleeding has a very limited influence on pneumonia, it should be practised early. With regard to M. Louis's results, it should be remembered, he states that all these patients enjoyed excellent health when they were attacked, and that the duration of the disease was estimated from the occurrence of febrile symptoms, up to the time when light food could be taken, which was generally three days after the fever had ceased.

That the result of an active antiphlogistic treatment was the production of a mortality of about 1 in 3 cases, seems to me further established by the account of Rasori,² who, in the great hospital of Milan, treated 648 cases by large doses of tartar emetic, of which 555 were cured, and 143 died, that is, 1 in $4\frac{1}{2}$. In publishing this statement, Rasori gives the result as one more favourable than the practice of blood-letting, which, of course, he would not have done unless the latter treatment was well known to have been attended with a greater mortality than that by tartar emetic, that is, 1 death in $4\frac{1}{2}$ cases.

M. Grisolle³ advocated more moderate bleedings than those so frequently had recourse to, his conscience preventing the abandonment of venesection altogether (p. 561). He analyses the 75 cases of Bouillaud, pointing out that only 49 were treated by the *coup sur coup* mode of bleeding, of which 6 died, or one in 8 cases, a favourable result, which he attributes to the youth of the patients treated. Of his own cases, one group of 50 cases was bled in the first stage of the disease only; of these 5 died, or 1 in 10. Those cases that

¹ *Recherches sur les effets de la Saignée.* Paris. 1835.

² From an Analysis of Rasori's Practice—*Annales de Therapeutique.* Janvier 1847.

³ *Traité pratique de la Pneumonie.* Paris, 1841.

died were bled most, each losing about 4 lb. 4 oz. of blood in successive bleedings. All the cases in this group were uncomplicated, and of the average age of 40 years. Of 182 cases that were bled in the second stage, 32 died, or more than 1 in 6. Here also those who died were bled most. Of the whole 232 cases, 37 died, that is about 1 in 6 $\frac{1}{3}$, as the general result of M. Grisolle's hospital practice, a mortality only one-half that of M. Louis's cases, although the circumstances under which they occurred were the same, with the exception of not being so heroically treated. Laennec also, who only bled moderately at the commencement of the disease, regarded the mortality to be 1 death in 6 or 8 cases.¹

Dr Glen, my present resident clerk, was so good as to tabulate for me all the cases of pneumonia given in the army returns, and reported by Colonel Tulloch.² Nothing can be more unsatisfactory than the nature of these returns, as we have no information as to the exactitude with which they were made, how the diagnosis was determined, or what was the treatment. The favourable mortality, as it has been supposed, of 1 death in 13 cases, which, according to Dr Glen, is the general result, is of little or no service to the present inquiry.

Treatment by Diet.—This treatment essentially consists in allowing the disease to go through its natural course. During the stage of fever the diet is light, and cold water allowed for drink; subsequently more generous diet is allowed, with wine, according to the nature of the symptoms. Sometimes a dietetic is converted into an *expectant treatment*, when remedies are given to meet occasional symptoms, as in the practice of Skoda, in the Charity Hospital of Vienna. An account of this has been given to us by Dr George Balfour of Cramond, who found from the books of the hospital, that during a period of three years and five months, commencing 1843, 392 patients were treated, of whom 54 died, or 1 in 7 $\frac{1}{4}$. Occasionally opium was given in small doses if there was much pain. Venesection was also practised early if there was much dyspnoea, and emetics given if the expectoration consisted of tough mucus.

Dr Balfour has also given some statistics of the Homœopathic Hospital of Vienna, accompanied however with statements which render it doubtful whether every case that applied was admitted, and consequently not fairly comparable with other hospital statistics. There can be no doubt, however, that many severe cases of pneumonia recovered under a system of treatment, which, it appears to me, most medical men must consider to be essentially a dietetic one.

Dr Dietl treated 380 cases of primary pneumonia, in the Charity

¹ *Forbes' Translation.* Fourth Edition. P. 337.

² *Government Statistical Reports on Mortality among the Troops, etc., 1853.*

Hospital of Vienna; 85 by venesection, 106 by large doses of tartar emetic, and 189 by diet only, with the following result:—

	Vene- section.	Tartar Emetic.	Diet.
Cured,	68	84	175
Died,	17	22	14
	<hr/> 85	<hr/> 106	<hr/> 189
Per cent., . . .	20·4	20·7	7·4
Deaths,	1 in 5	1 in 5·22 . . .	1 in 13½

It was further observable, that of the 85 cases treated by blood-letting, 7 of the fatal cases were uncomplicated; whilst of the 189 cases treated by diet, not one of the deaths was an uncomplicated one.

Treatment directed to further the natural progress of the disease.—The treatment I have pursued in pneumonia is founded on the pathological principles formerly given, viz., never to attempt cutting the disease short, or to weaken the pulse and vital powers, but on the contrary to further the necessary changes which the exudation must undergo, in order to be fully excreted from the economy. To this end, during the period of febrile excitement, I content myself with giving salines in small doses, with a view of diminishing the viscosity of the blood. As soon as the pulse becomes soft, I order good beef tea and nutrients; and if there be weakness, from 4 to 8 ounces of wine daily. As the period of crisis approaches, I give a diuretic, generally consisting of ℥ss of nitric æther, sometimes combined with m℥ of colchicum wine, three times daily, to favour the excretion of urates. But if crisis occurs by sweat or stool, I take care not to check it in any way.

On examining into the results of this practice, which has been publicly carried on by me in the clinical wards of the Royal Infirmary during the last eight years, and which has been carefully recorded by the clinical clerks, I find the total number of cases to be 65; the average age, 31 years.

Of these, 62 were dismissed cured, and 3 died; that is, one in 21½.

Of the 62 cases cured, 55 were uncomplicated, and 7 complicated. Of the 55 uncomplicated cases, I find that the clerk has omitted to state either the exact day of rigor or of convalescence in 4, so that no deduction can be derived from them, as to the duration of the disease. But of the remaining 51 uncomplicated cases, 40 were single and 11 double pneumonias.

The duration of the 40 cases of single pneumonia was as follows, viz.:—3 cases recovered in 7 days; 2 cases in 8 days; 4 cases in 10 days; 1 case in 11 days; 3 cases in 12 days; 2 cases in 13 days; 9 cases in 14 days; 1 case in 15 days; 3 cases in 16 days; 2 cases in 17 days; 3 cases in 18 days; 1 case in 19 days; 1 case in 20 days; 2 cases in 21 days; 1 case in 22 days; 1 case in 23 days; and 1 case

in 26 days. Average duration of single uncomplicated pneumonias, $14\frac{1}{2}$ days.

The duration of the 11 cases of double pneumonia were as follows: 1 case recovered in 13 days; 2 cases in 14 days; 1 case in 16 days; 2 cases in 18 days; 1 case in 20 days; 3 cases in 21 days; and 1 case in 55 days. Average duration of double uncomplicated pneumonias, 21 days.

Of the 55 uncomplicated cases, 6 were bled and were subjected to an antiphlogistic treatment before admission. Of these, 1 case recovered in 7 days; 2 cases in 14 days; 1 case in 16 days; 1 case in 17 days; and 1 case (a severe double one) in 55 days. Average duration of cases bled, $20\frac{1}{2}$ days.

Of the 7 complicated cases of pneumonia which recovered, 1 case supervened on chronic asthma, bronchitis, and emphysema, and recovered in 14 days;

1 case supervened on typhus fever, and recovered in 16 days;

1 case supervened on chronic asthma, bronchitis, and pleurisy, and recovered in 48 days;

1 case supervened on typhus fever, and recovered in 18 days;

1 case supervened on pleurisy on one side, with pleural exudation existing 8 weeks before admission, and recovered in 19 days;

1 case supervened on rheumatism with heart disease, and recovered in 19 days; and

1 case supervened on very severe rheumatism, with endo- and peri-carditis, but recovered in 15 days.

The average duration of the pneumonia in the seven complicated cases was $21\frac{1}{3}$ days.

The three fatal cases were all complicated. The first, with uncontrollable diarrhoea; and, on dissection, conjoined with pneumonia there was found extensive follicular disease of the mucous membrane of the duodenum, jejunum, but chiefly of the ileum. The second case was complicated with persistent albuminuria and anasarca. No *post-mortem* examination could be obtained. The third case, that of a drunkard, was complicated with delirium tremens, and latterly violent convulsions. On dissection, in addition to the pneumonia, there was found universal cerebral meningitis, with exudation, at the base, as well as over both hemispheres of the brain.

In addition to the three fatal cases here recorded, I have found in the pathological registers kept by Drs Gairdner and Haldane seven other cases, in which, as the result of chronic, cerebral, cardiac, renal, or other pulmonary disease (such as phthisis), pneumonia appeared before death, adding a fatal complication to previously existing maladies. Not one of these can properly be considered as a case of acute pneumonia, or indeed of pneumonia at all. They have all been entered by the clerks in the ward books as softening of the brain, morbus cordis, Bright's disease, or other lesion for which the patients

entered the Infirmary and were treated. In most of them it was the *pneumonie des agonizans* of the French.

These, then, are positively all the cases of acute pneumonia which have entered the Infirmary under my care during the last eight years, so far as I can discover them. Last winter I read through, analysed, and tabulated them myself. During the recent Christmas vacation I again went over them with Dr Glen, my present clinical resident physician, to whom I am much indebted for the great care and pains he has taken in confirming these results. Every case has been treated publicly, and is open for inspection in the ward books; and the result is as I have stated, that the mortality of the acute pneumonias, in the practice of the clinical wards, while under my care, is 1 in $21\frac{2}{3}$, and that of all the cases of uncomplicated pneumonia, 55 in number, not one has died, although many of them have been very severe, involving the whole of one lung, and in 11 cases portions of both lungs.

So far, I think, I approach very near correctness by saying that the result of a vigorous antiphlogistic treatment of pneumonia, as formerly practised, is a mortality of 1 in 3 cases; that the result of a treatment by tartar emetic in large doses, according to Rasori, and more recently to Dietl, is a mortality of 1 in 5 cases—but according to Lacnec, 1 in 10 cases; that the result of moderate bleedings, as in the treatment of Grisolle, is a mortality of 1 in $6\frac{1}{2}$ cases; and that the result of a dietetic treatment with occasional bleedings and emetics in severe cases, as with Skoda, is a mortality of 1 in 7, and if pure, as under Dietl, a mortality of 1 in 13 cases, all carried on in large public hospitals. Further, that the mortality from pneumonia in the army and navy, occurring generally among healthy able-bodied men, is also a mortality of 1 in 13 cases. Lastly, that the result of a treatment directed to further the natural progress of the disease as I have explained it, is, in the clinical wards of the Royal Infirmary of Edinburgh, when under my care, up to this time a mortality of 1 in $21\frac{2}{3}$ cases.

From these facts, it follows that uncomplicated pneumonias, especially in young and vigorous constitutions, almost always get well, if instead of lowering the vital powers they are supported, and the excretion of effete products assisted. It is exactly in these cases, however, that we were formerly enjoined to bleed most copiously, and that our systematic works even now direct us to draw blood largely and repeatedly in consequence of the supposed imminent danger of suppuration destroying the texture of the lung. Such danger is altogether illusory, and the destruction to lung tissue, so far from being avoided, is far more likely to be produced by the practice. In fact, the only cases in which it occurs are in aged or enfeebled constitutions, in which nutrients, and not antiphlogistics, are the remedies indicated. We can, however, readily understand how blood-letting, practised early, and in young and vigorous con-

stitutions, does less harm, or, to use a common expression, "is borne better," than when the disease is advanced, or the patient weak, and this because then the vital powers are less affected by it. Hence the diminished mortality in the second series of Louis's cases, and probably in the army and navy cases. But that it cures the greater number of persons attacked, or shortens the duration of the disease, is disproved by every fact with which we are acquainted.

At the same time there are cases, which were formerly often mistaken for inflammation, in which blood-letting may still be useful. I allude to those where an obstruction to the circulation exists in the heart and lung, dependent on over-distension of the right side of the former organ, and cases of venous congestion, engorgement, and perhaps œdema of the latter; also certain cases of bronchitis preventing aeration, of aneurisms, and of asphyxia. Although even here the true value of the remedy has yet to be positively ascertained, the special cases demanding it more carefully discriminated, and the mechanical principles which justify the practice determined.

The temporary benefit occasioned in many of these cases by the loss of a trifling amount of blood is often very remarkable, and has been previously referred to (p. 783). I have seen instances where great dyspnœa and pain, caused by large thoracic aneurisms in vigorous men, have been greatly alleviated, and inexpressible relief produced for from twelve to twenty-four hours, by a bleeding to the extent of only five ounces. It seems probable, that this may arise from diminishing for a time the tension of the whole vascular system. But whatever be the explanation of this fact, I hold that, as a palliative, and practised to a limited extent in cases where no great debility exists, blood-letting may still be had recourse to. So with regard to antimonials, although in the large doses, which weaken the heart and force of the pulse, they are not serviceable—in smaller doses, together with other neutral salts, they may assist in diminishing the viscosity of the blood, and in favouring the excretion of the effete matters by the skin and kidneys.

As to mercurials, the confident belief in their power of causing absorption of lymph, by operating on the blood, is not only opposed to sound theory, as formerly explained, but, like blood-letting, is not supported by that experience which has been so confidently appealed to in their favour. They have been most praised in the treatment of serous inflammations and in iritis. But recent careful observation has demonstrated that the moment these diseases are treated without mercury they are uninfluenced (except in certain cases for the worse) by this drug. Thus, from an analysis of 40 cases of pericarditis, recorded with unusual care by the late Dr John Taylor, only 4 appear even coincidently to have benefited in any way.¹ And of 64 cases of iritis, of every degree of severity, including its idiopathic, traumatic,

¹ *British and Foreign Medical Review*, vol. xxiv., p. 565; and *Lancet*, May 1845 to October 1846.

rheumatic, and syphilitic varieties, treated without mercury, by Dr H. W. Williams of Boston, U.S., the results—with four exceptions, which were neglected at the commencement—were perfectly good.¹

I cannot, therefore, resist the conclusion, that the principles which led to an antiphlogistic practice in acute inflammations were erroneous, and are no longer in harmony with the existing state of pathology. I think it has been further shown, that in recent times our success in treatment has been great, just in proportion as we have abandoned heroic remedies, and directed our attention to furthering the natural progress of the disease. Thus, in our large public hospitals, under circumstances pretty much the same, it has been shown that the mortality of pneumonia has been diminished from 1 in 3 to 1 in 7 cases, then to 1 in 13, and lastly, to 1 in 21 $\frac{2}{3}$ cases. In other words, death in this disease takes place seven times less frequently now than it did twenty years ago.² This great improvement in practice, it appears to me, is attributable—1st, To the greater accuracy with which we can now detect inflammations of the lung; and 2d, To our better acquaintance with their pathology—and the result is not the less certain with men of experience, because these causes operate insensibly to themselves. How often, during the last sixteen years, have we been asked, of what use are your stethoscopes, your microscopes, and your chemical analyses at the bedside? In reply, we point to the revolution now going on in the practice of medicine, to the establishment of scientific laws instead of empirical rules, and to the abandonment of a palliative in favour of a curative plan of treatment.

ARTICLE II.—*On the Use of Chloroform in Midwifery Forceps Operations.* By J. MATTHEWS DUNCAN, A.M., M.D., F.R.C.P.E., Lecturer on Midwifery, Physician-Accoucheur to the Royal Dispensary.

(Read before the Medico-Chirurgical Society, on the 4th Feb. 1857.)

ON the introduction of anæsthesia, by chloroform, into the practice of midwifery, numerous objections to its use were urged by various parties, on various grounds. None of these appeared then to have more force than that adduced by Professor Meigs of Philadelphia, who maintained that the patient's retention of sensibility to pain—especially during the application of the forceps—was a very important aid to the obstetric surgeon, in securing her safety generally, and specially the integrity of the passages. This objection of Professor Meigs was answered, chiefly on theoretical grounds, in a published letter by Professor Simpson. That answer was as able

¹ *Boston Medical and Surgical Journal*, 1856.

² I am satisfied, also, that deaths from acute pericarditis are far less common now than formerly, and that *post-mortem* examinations, as a consequence, demonstrate adhesions of the pericardium much more frequently.

